

# SENSORY INTEGRATION AND COGNITIVE THEORY

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The question of whether a multiplicity of processes must be postulated in order to account satisfactorily for the phenomena of animal learning poses two independent but related problems. The first is a phylogenetic problem for which the facts of comparative anatomy suggest a clear answer. Since the behavior of the different animal species is modified through the operation of different organic systems, the question, from the phylogenetic viewpoint, is no longer *whether* a multiplicity of processes exists but *what* are the characteristics of the diverse processes that make learning possible at the various levels of animal life. The second problem is intra-specific rather than inter-specific. It is concerned with the nature of the processes which underlie learning in any given animal form. This paper, then, is divided into two sections. In the first we shall deal briefly with some of the implications of comparative psychology for learning theory, while in the second we shall be concerned with the intra-specific problem. We have chosen to consider cognitive theory in our discussion because we feel that it provides an excellent vehicle for the analysis of both aspects of the multiple-process problem.

There can be little doubt that the most influential criticism of a pure reinforcement theory of learning has been provided by the cognitive theorists under the leadership of Professor Tolman (15). The effectiveness of this criticism is revealed both by the amount of

experimental work which reinforcement theorists have devoted to attacking the weaker aspects of Tolman's position and by the silence they have maintained concerning some of the most crucial items of evidence adduced by the cognitive theorists. However, although we are indebted to Professor Tolman for repeatedly calling the attention of psychologists to phenomena of learning which stimulus-response psychologists have tended to overlook, we believe that his systematic point of view shares a vital weakness with reinforcement theory. Both positions are anti-comparative, functionally if not explicitly, in that they lead to formulations which are so gross as to be applicable to all animal forms—not only to rat, the favorite subject for investigation, but to paramecium and to man. True, Professor Tolman's occasional reference to "capacity laws" may be regarded as a bow in the direction of phylogenesis, yet it is difficult to see why the educated paramecium of Day and Bentley (4) could not be characterized, in Tolman's terms, as a conscious, purposeful creature which develops cognitive maps (however narrow) and acquires gestalt-expectations. In Professor Hull's terms, of course, changes in the behavior of the trained paramecium might be thought of only as another instance of the strengthening of stimulus-response connections under the influence of need-reduction.

The adoption by both theorists of what has become known as the molar

viewpoint leads inevitably to the blurring of phylogenetic distinctions. It is sometimes useful in the preliminary stages of research to think of behavior in functional terms which make no immediate reference to underlying bodily processes, but it is inexcusable to cling doggedly to gross formulations which are based upon the end results of behavior and which are permanently divorced from underlying mechanisms. The processes of modification in paramecium and rat are not qualitatively identical because the two animal forms are anatomically quite dissimilar. Any theoretical approach which glibly obscures this difference should be recognized as the most dangerous variety of subjectivism. The so-called molar view is sometimes justified in the name of parsimony, but this clearly is to misunderstand the meaning of the concept. In the case of the paramecium—to return to our rather extreme example—the superficiality of both molar formulations is demonstrated in an experiment suggested by Buytendijk (3). Preliminary immersion of a naive control animal in a solvent renders it just as capable of escaping from the capillary tube as an experimental animal patiently practiced after the manner of Day and Bentley. This finding makes it clear that improvement in the trained animal may be the product of increased flexibility resulting from activity-produced chemical changes in protoplasmic constituents, and may have no relation to the specific stimulating conditions encountered in training, or to the nature of the ends attained by the responses which terminate each training trial. Theoretical frameworks of the molar sort not only provide no pegs on which to hang such findings, but tend to discourage the fundamental research which produces them. Here is parsimony by exclusion—by exorcism.

This common failing of cognitive and

reinforcement theory must not be obscured by the fact that the two formulations are couched in very different language. Professor Tolman likes to talk in phenomenal terms frankly borrowed from the introspectionists, while Professor Hull prefers a dialect of the Watsonian language. Certainly Professor Tolman's formulations are dangerously anthropomorphic, but in a fundamental sense so also are those of Professor Hull. Anthropomorphism is dangerous not because it humanizes animals, but because it encourages us to be content with conceptions which are nothing more than loose analogies and which therefore obscure real differences among phylogenetic levels. Hullian theory falls into the same error. Only the *kind of analogy* upon which it is based is different.

A psychology without mind may be conceivable, but a psychology without mind *or* body is not. If we take the facts of evolution seriously, then the answer to the first question before us is clear. There is not one learning process, but many. We should not be at all surprised to find qualitatively distinct processes at every major level of anatomical organization. Certainly we may expect to find general processes which are essentially similar over a wide range of organisms in the phylogenetic series, but we cannot be content until we have found differences in functional capacity consonant with the wide differences in structural organization which confront us.

The history of learning theory shows an ever increasing gap between the phenomena which are presumably under consideration and the theoretical systems that have been constructed to deal with them. It is only living organisms that learn, and any systematic discussion of learning must, if it is to remain relevant, refer to the characteristics of these organisms in all their similarity

and diversity. Behavior which characterizes the rat may be quite atypical of the earthworm, and even the briefest glance at man demonstrates his difference from the stereotyped ant or the lowly amoeba. Operational ease cannot be substituted for the hard digging of science, and the ubiquitous white rat is an inadequate model upon which to base our conceptions of the psychological natures of all other organisms. In the concentrated and often heated activity of system making, the simple truth that organisms exist at different phylogenetic levels has been lost, and with it the essential problem for learning theory—the search for the mechanisms which make behavioral modification possible.

When we turn from the broad phylogenetic problem to an inquiry into the nature of modification in any given animal form, such as the rat, similar considerations are relevant. Here we no longer have so strong a reason to anticipate the discovery of a multiplicity of qualitatively distinct processes rather than a single basic process, but once again it may be well to proceed cautiously. Professor Tolman has suggested that "our familiar theoretical disputes about learning may *perhaps* . . . be resolved, if we can agree that there are really a number of different kinds of learning. For then it may turn out that the theory and laws appropriate to one kind may well be different from those appropriate to other kinds. Each of the learning theories now current may, in short, still have validity for some one or more varieties of learning, if not for all" (16, p. 144). He then goes on to recognize the validity of reinforcement theory with respect to the acquisition of "positive cathexes" (although he hastens to add that there is no good evidence for the view) and, in the matter of "motor patterns," allows Guthrie to win by de-

fault. In our opinion this genial eclecticism, although in many respects admirable, misses the important point. While it is true that each of the major theories has tended to focus on limited aspects of learning, we can by no means be sure that any given theory deals adequately even with those phenomena with which it has been most directly concerned. The *data* obtained in experiments conducted by adherents of the various schools must be given respectful consideration, but it does not follow that the *explanations* advanced to account for these data can be accorded equivalent status and set side-by-side to make a comprehensive theory. Lumping together the reports of the seven blind men does not produce an adequate description of the elephant.

In a recent paper we have defended the position that at least two qualitatively distinct learning processes must be postulated in order to deal adequately with available data on the learning of lower mammals (1). On the one hand there is the organization and reorganization of motor patterns which occurs within the confines of a given environment. The animal selects, refines, and fixates those responses which are appropriate to the relationship which exists between internal and external conditions. This is the problem with which reinforcement theorists have been almost exclusively concerned, and the evidence to some extent justifies their theoretical emphasis on afferent-efferent integrations and the role of biological satisfactions. But we are still very much in the dark with respect to the nature of the process or processes which make selective learning possible. Hull's fourth postulate (6) does not begin to deal with this problem, but in fact avoids it. Not many years ago learning theorists were apt to pose a fundamental question relating to the empirical law of effect. In what way,

they asked, do the satisfying consequences of a response work back upon the organism to increase the likelihood that the response will appear on subsequent contacts with the situation? Unfortunately, in this heyday of modern positivism, such questions have become exceedingly unpopular.

The data of conditioning experiments, on the other hand, suggest the operation of a second process which seems much easier to understand. Following Maier and Schneirla (8) we have chosen to refer to it as a process of sensory integration. When two afferent centers are contiguously activated, a functional relation is established between them such that the subsequent innervation of one will arouse the other. Here we postulate a purely afferent process of modification which may operate not only in the absence of concurrent motor activity but in the absence of need-reduction as well. This process is revealed most directly in the development of stimulus-equivalence which occurs in conditioning—when two stimuli are presented contiguously, the first acquires some of the functional properties of the second. It also helps us to understand many of the phenomena which occur in other learning situations, especially those phenomena which, as the cognitive theorists have noted, cannot be dealt with adequately by Hullian theory. It should be recognized, of course, that the concept of sensory integration has long been implicit in the cognitive formulations. Some years ago Professor Spence (12) made the point clearly when he labeled Professor Tolman's position an "S-S Contiguity" theory.

In criticism of stimulus-response-reinforcement theory, the cognitive studies reiterate one essential point, namely, that animals may respond appropriately under changed motivational or environmental conditions even though the re-

sponses elicited are not identical with those previously practiced. This means that in the course of learning integrations are developed which cannot easily be understood as the attachment of responses to stimuli. However, cognitive theorists have not been concerned with the mechanisms underlying such behavior, but have contented themselves with phenomenal analogies to a presumptively understood human consciousness.

The important studies of Blodgett (2) and Tolman and Honzik (18), which clearly demonstrate that the rat learns a great deal about a maze under conditions which preclude the reinforcement of specific response patterns, have provided the strongest support for the cognitive position. That the data of these experiments pose an extremely difficult problem for stimulus-response-reinforcement theory is attested by Hull's failure even to mention them in his *Principles of Behavior*, and by the scant attention which they receive from S-R theorists in papers on the more recent, though directly related, researches with the T-maze. In our opinion the principle of sensory integration makes it possible to understand these results.

It might be profitable to ask once more the familiar question of how a rat learns to take the true-path turn, rather than enter the cul, at a choice-point in a maze. The traditional answer of response-reinforcement theory is about as follows: The complex cues (interoceptive as well as exteroceptive) which are present at the choice-point become individually, and as a group, connected to the response of turning in the correct direction on the basis of ensuing reinforcement, primary or secondary. But the concept of sensory integration suggests an alternative interpretation. From this point of view, not one, but two stimulus configurations may function at the choice-point, at least in the early stages of learning—one arising from the

mouth of the cul and the other from the entrance to the true path. When the animal takes either path a succession of new afferent patterns arise, in the course of movement, with which the entrance-pattern (directly or mediately) becomes functionally equivalent. For example, when the animal enters a cul the entrance-percept is followed sooner or later by a characteristic obstruction. If the blind is both short and homogeneous, the closely contiguous relation between entrance- and end-patterns gives rise to a process of afferent integration and the equivalence is readily established. When motivational conditions are such that avoidance-behavior would be evoked by the cul-end, this behavior will now be evoked by the cul-entrance. In longer and less homogeneous blinds, such as those which involve a number of turns, there is a succession of critical percepts (principally at the elbows of the cul), and end- and entrance-percepts are less immediately related. Under these conditions cul-elimination proceeds in stepwise fashion, backwardly from end to entrance, with the units of blind-alley pathway usually being eliminated as wholes. From our point of view, the less rapid elimination of long blinds is explained, not in terms of a temporal gradient of reinforcement, but in terms of the temporal relation between stimulus patterns arising at cul-entrance and cul-end. The great difficulty which is presented by circular blinds is to be understood from the fact that they provide no characteristic barrier-percept. The principle of sensory integration also leads to the assumption that choice-point learning in the early sections of a complex maze is to be understood, at least in the initial stages of learning, not in terms of the development of true-path preferences, but rather as a process of cul-avoidance, since the afferent consequences of correct turns are less structured and less directly significant than those of cul-

entry. The true path units do, of course, eventually acquire more positive properties, but in a more gradual fashion due to their remote relation to the goal-object, and the very rapid learning which occurs at later points in the maze can be interpreted as a joint function of integrations which develop with respect both to cul- and true-path entrances. It might also be suggested that "latent" learning in a complex maze is largely confined to the region of the culs, as an experiment by Reynolds (10) has indicated, and to the goal-box which has a rather special afferent character derived from the removal of the animal from the maze.

In order to deal with serial learning in terms of reinforcement theory, Hull is forced to rely heavily on the principle of secondary reinforcement which bridges the temporal gap between early responses in the series and the terminal "primary" need-reduction. According to this principle, which is regarded as a corollary of the fourth postulate, stimuli which arise in the course of running not only become guides to action, in the sense that they are connected to appropriate responses, but they—the same stimuli—also acquire goal properties which are mediated in a backward direction. In some respects this hypothetical process resembles our own conception of the manner in which true-path units acquire positive properties, but there are important differences. Hull maintains that the development of secondary reinforcement is itself a process of stimulus-response connection, in which the stimuli in question become attached to "fractions" of the response patterns involved in need reduction. Afferent relations are not, in this view, established directly, but must always be response-mediated, and these relationships can only be established when one of the stimuli itself arouses need-reducing responses.

Now it is our position that all of the

data with which Hull deals in terms of the concept of secondary reinforcement can be dealt with equally well if one assumes a process of sensory integration in which relations between afferent processes, or sensory equivalences, are established, not indirectly, but *directly*, in the course of contiguous arousal, and which may occur in the absence of drive-decrement. Furthermore, this assumption makes it possible to account for the classical latent learning data, while Hull's conception does not. From our point of view the latent learning experiment may be understood as a complication of the sensory pre-conditioning experiment. It must be emphasized that not only a contiguity principle, but an *afferent* contiguity principle, is required by this evidence. Neither the version of Professor Guthrie (5) nor the version of Professor Mowrer (9) will do.

In recent years cognitive theory has stimulated a variety of experiments with the T-maze, which are closely related to the latent learning problem, and which point even more clearly to a sensory integration process. For the most part, these experiments may be grouped under three major headings. In all three the rat is trained in the maze, food being situated in one arm and water in the other, and later tested under motivation which corresponds to one or the other of these incentives. In the first type of experiment the animal is satiated for both food and water during training and in the second type of experiment the animal is both hungry and thirsty during training. The results of both types of experiment show a significant tendency for the rats to respond appropriately when singly motivated—hungry or thirsty. Even if reinforcement operates in both experiments—secondary reinforcement may be presumed to operate in the first—the forced-turn procedure employed in training should keep the alternative response-tendencies

approximately equal in strength, and the animal should respond randomly. Even if turning tendencies of unequal strength become established—due perhaps to the greater strength of one or the other of the drives or to the greater potency of one or the other of the reinforcements—the animal cannot be expected to respond appropriately under *both* conditions of single motivation in the test trials. The *ad hoc* principle of the selective association of drive-stimuli (7) may account for the results of the second type of experiment, but it will not account for those of the first type in which there are no drive stimuli to be selectively associated. The last resort for the reinforcement theorists is the untestable concept of unobservable anticipatory response (13, 17).

In a third major type of experiment stimulated by cognitive theory, the animal is trained under one drive and tested under the second. Here the animal fails to respond appropriately, and the reinforcement theorists, as might be expected, have made much of this fact. A recent experiment by Walker, Knotter, and DeValois (19) suggests, however, that the forced-turn procedure ordinarily employed may in great measure be responsible for the negative results. Trained under conditions of water deprivation, with water in one goal box and both food and water in the other, their animals did respond appropriately when the drive was shifted to hunger. In another variation of the basic design Strange (14) found clear evidence of latent learning under single drive conditions.

In conclusion, there are two points that we should like to make. First, there is a good deal of other evidence which, in our opinion, can be better understood in terms of the concept of sensory integration than in terms of either cognitive or reinforcement theory. Our purpose here was only to show how the idea of sensory integration which was

developed originally to deal with the results of conditioning studies could be applied to behavior in other standard learning situations. Secondly, we should like to note that the sensory integration postulate is regarded, not as a final solution to our problems, but as a basis for further research. What conditions other than contiguity of stimulation affect the integration process, and in what manner? Do the afferent consequences of motivational states play a special role? It seems that they do, although it is likely that the *onset* and *presence* of these afferents are just as significant as their *decline* and *disappearance* (11). Above all, there is the question of how an understanding of sensory integration will make it possible for us to deal with the problem of selective learning. Traditional stimulus-response theory is based on the assumption that any stimulus may be tied to any response. But suppose we say instead that, not stimuli, but afferent organizations or afferent integrations produce characteristic patterns of response, and that for each pattern of afferent organization there can be one, and only one, pattern of response. It follows directly from this formulation that the process of selective learning can be considered, not as a process of stimulus-response connection, but as a *process of achieving the afferent organization required to produce a given pattern of response*. Here at last we may have a meaningful, process-oriented translation of the dictum of the cognitive theorists: Given knowledge, behavior will take care of itself.

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